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Ocean Science Discussions

Interactive comment on "A new method for forming approximately neutral surfaces" *by* A. Klocker et al.

Anonymous Referee #1

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Review of Klocker et al

The paper presents a new proposal for the construction of approximately neutral surfaces in the ocean. Different from earlier attempts by two of the present authors, the guiding principle is to minimize the overall rms slope difference between the approximate neutral surface and the neutrally tangent plane (ntp). This procedure also minimizes the fictitious diffusion arising as a result of isoneutral mixing along the ntp. After discussing the method in some detail, the new approximate neutral surfaces are computed from model results. The main result of the paper is to show that the new surfaces indeed achieve smaller fictitious diffusion than the previous neutral surface approximation (gamma_n) of the author group, and much smaller than the isosurfaces of several other density variables discussed in the literature. The paper is on the high technical

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level expected from the senior author.

My only reservation stems from the fact that all results are based on the hydrographic fields from the output of a standard MOM4 run, i.e., not on observations. This is a pity since the main application of neutral surfaces - as mentioned on p.441, l.21-25 - is in water mass analysis and inverse models. Those applications make less sense with model output since models normally have much higher spurious diffusivities than the 0.1 (in cgs-units) considered here as physical value. If the paper's main findings could be based on observed hydrography or atlas data, they would be much stronger and of more interest to Ocean Science readers. The remark on p.433, l.15 suggests that corresponding computations may already have been done, so this might not be an unduly request.

With those and further changes as suggested below, I recommend publication.

Further comments:

p.424, l.11-14: While isopycnic layer models do have errors related to the choice of potential density level, by construction they do not have fictitious mixing. Such mixing only occurs when potential density surfaces are used for analyzing hydrographic data.

p.426, l.6 f.: It would be helpful at this point to give a comparative discussion of the principle used for construction of gamma_n.

p.428-430: The authors have done an excellent job in relegating technical material to various appendices. To facilitate reading for the non-expert ocenographer, the part of section 3 dealing with the discrete implementation of the minimum principle, and with the methods for finding the minimum, should also be put in an appendix.

p.434, l. 6 f.: Does the choice of one depth range (around 600 db) influence the results? Results from other depth ranges should be mentioned here.

p.438, l.24: Language: How can an equation be minimized?

OSD

5, S197–S199, 2008

p.439, section 4.4: The physical meaning of a combination of potential density and steric anomaly remains obscure. Suggest to drop this section.

p.444, appendix C: Suggest to drop, at least this reviewer already believes in Stokes' theorem, both in 2 and 3 dimensions.

Figures: The paper would gain from a certain reduction in the number of figures, which can be achieved without loss of message.

Fig. 2 confirms that eq. (11) indeed holds to good approximation. Since only the small pressure dependency of haline contraction has been ignored in deriving (11), this does not need to be shown. Suggest to drop.

Fig. 7 is another form of showing the balance in eq. (11). The scalar variable is already shown in fig. 4c. Suggest to drop.

Figures 10 - 12: The diagrams in fig. 13 and 14 are much more effective in communicating the essential properties of the different density variables than the color plots in figures 10-12 which look rather repetitive. I suggest to drop figures 10-12, and give information for all variables in the form of fig. 13. If the authors want to give more information, they might consider repeating fig. 14 for one or two different depth ranges.

Interactive comment on Ocean Sci. Discuss., 5, 419, 2008.